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with the Respects of the Author

ON THE

PENETRATIVENESS

OF

FLUIDS.

BY J. K. MITCHELL, M. D.

LECTURER ON MEDICAL CHEMISTRY IN THE PHILADELPHIA MEDICAL INSTITUTE.

Extracted from the American Journal of the Medical Sciences, for November, 1830.

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IN 1829, I read before the Philosophical Society, a short memoir on a new method of forming gum elastic into thin plates, sheets, and bags. In some instances the balloons formed by the process then described, had, when filled with hydrogen gas, the power of ascending to a considerable height in the atmosphere. Those which were confined to the atmosphere of my lecture room, at the Medical Institute, descended again after a period of time, varying from an hour to two days. The cause of the descent, which did not seem of easy explanation, became a subject of investigation.

The gas might have escaped from the balloons at the ligature, or by permeating the dense wall of gum elastic, or by uniting chemically with the internal surface of the latter. To free the gas from the compression to which it is subjected in a balloon, I confined it in a wide-mouthed bottle, over the aperture of which I tied very firmly a thin sheet of the elastic membrane. In a few hours the descent of the cover into the cavity of the bottle gave evidence of a diminution of the contained gas, and finally the cover was burst inwards by the pressure of the atmosphere, so great had been the rarefaction of that which remained in the bottle. On weighing the membranous cover, no gain in weight could be perceived, so that I presumed that the gas had escaped. By repeating the experiment, and covering the bottle with a small bell-glass holding atmospheric air, I found, after a time, in the latter vessel, an explosive mixture, while the contents of the bottle itself were found to be pure or nearly pure hydrogen. Evidence was thus afforded that hydrogen penetrated the membrane not by any *vis a tergo*, for no pressure was applied, but by some inherent power of considerable amount. The facility of permeation appeared also much greater in the hydrogen than in the atmospheric air, which, if it entered at all into the bottle, did not penetrate in any appreciable quantity, when fully one-half of the hydrogen had made its escape.

In the next experiment the arrangement of the gases was altered: common air was enclosed in the bottle, and a bell-glass confined around it an atmosphere of hydrogen. As was expected, the hydrogen entered the bottle rapidly, raised up the tense membrane, formed it into a globe, and finally burst through it, and thus made its escape from the confinement to which it had been spontaneously subjected.

The minuteness of the atom of hydrogen might readily enough account for the greater facility with which it penetrated the membrane, but could not be considered a good reason for the *energy* with which the penetration was accomplished. A gas having a heavy atom was therefore selected for further experiment, and carbonic acid, subjected to the same sort of confinement was found to permeate the membrane with as great power, and very much greater facility. In succession, most of the gases were submitted to the same ordeal, and all of them found, except nitrogen gas, to exercise the same power, but with very different degress of rapidity. The *power* was ascertained by comparison with common air, and the *rate of action* both in that mode and by comparison with each other. The depression or elevation of the membranous cover, clearly indicated the escape or entrance of a gas, and when two active gases were placed one on each side of it, its rise or fall expressed the difference of rate, because each was, at the same moment, in the act of permeation, as proved by many examinations of the contents of the bottle and bell-glass.

Having once ascertained the rate of action of each gas relative to air, a prediction could be made as to their rate in reference to each other. Hence gases which operated on air with nearly equal velocity, affected the *horizontality* of the membrane very little when placed on opposite sides of it. Thus carbonic acid and nitrous oxide act with great facility on common air, and in nearly equal degree; and when placed on opposite sides of the membrane, penetrate it rapidly, but cause a very slow change in its position. The facts here presented warrant the conclusion, that if two gases, equally penetrant *exactly*, could be found, they would under the above described arrangement, mix uniformly, without in the slightest degree altering the state of the membrane.*

* Subsequently having discovered that olefiant gas and arsenuretted hydrogen, have, with reference to common air, exactly equal rates, they were placed

The greatest possible degree of effect on the membrane arises, when we place on opposite sides of it, the slowest and most speedy penetrator; for instance, nitrogen and sulphuretted hydrogen. In that case the change is immediately visible.

As in all the previous experiments, different gases were placed in comparison, I placed the same gas on both sides, and expected, for the "sufficient reason," no change. The experiment accorded with expectation. The membrane remained stationary.

The circumstances *essential* to the transmission of gases through the membrane, formed an interesting subject of inquiry.

My first attempt was to produce a vacuum, by placing the gas in a bottle, and exhausting by means of the air-pump, the bell-glass which covered it. The gases effected their escape from the bottles thus treated, with a velocity proportional to the rate of permeation already ascertained; sulphuretted hydrogen passed out more rapidly than carbonic acid, and that than hydrogen. Still as *some* air is always found in an exhausted receiver on the finest air-pump, I passed a tube containing carbonic acid into a Torricellian vacuum where it very speedily escaped and caused the descent of the mercury. Even this experiment could not prove perfectly satisfactory, as mercurial vapour occupies the barometric vacuum. A perfectly empty bag carefully closed was placed in carbonic acid and nitrous oxide successively, without undergoing the slightest inflation. If a very small portion of *any kind* of air remained in the bag, inflation followed, provided the bag were exposed to a *different* gas.

By another arrangement I obtained my object more unexceptionably. Having found by inverting a bottle holding confined gas, and thus plunging it into mercury, that no gas escaped, and that consequently mercury could not promote or sustain the permeation of the gas, I reached my object by the following means. Closing a tall cylindrical lamp-glass at *one* end with gum elastic, and filling it with mercury, it was placed, so filled, on the shelf of the mercurial trough, having the end closed by the membrane uppermost. Through this fine film the mercury could be plainly seen in close contact with its under surface, while the deep depression of the membrane showed the power of the column of mercury by which it was drawn down. By leaving it in the air, or by placing over it a bell-glass of any gas, more slowly, but at their settled rates, the gases penetrated the membrane and accumulated in the cylinder, thus permitting the de-

on opposite sides of a membrane, with the full expectation of sustained horizontality on the part of the membrane; which was confirmed by the result.

scent of the mercury. The process continued long after the mercury had abandoned the surface of the membrane, and the space was occupied by the gas, in, of course, a rarefied state.

It became then evident, that any thing which could remove the gas from the surface of the membrane at which it had arrived by penetration, would continue its transmission. Of course then agents chemically attractive of a particular gas, when placed beneath the membrane would promote its permeation. In fact, lime water and solution of baryta were rapidly carbonated by the transmission of carbonic acid, and sulphuretted hydrogen almost instantly precipitated the lead of the acetate placed in solution on the opposite side of the membrane, which became black on the side of the solution. A neater mode of performing this experiment is the following. Inject by means of a gum elastic bottle and pipe, into a very small bag of gum elastic, stretched until fully transparent, a solution of the substance to be acted on. Carefully tied, washed, and dried, the bag is to be passed up through mercury into a receiver holding the gas, which for solution of baryta should be carbonic acid, and for that of acetate of lead, hydrosulphuric acid. In a few moments, in the former case, a white coat is seen to completely line the internal surface of the bag, and in a few minutes to fall down and accumulate at the bottom of it. In the latter case, the inner coat is dyed indelibly black. In either case if water be alone placed in the bag, it will absorb a considerable quantity of either of these gases, and their presence may be ascertained by the usual tests.

If any suspicion had arisen that the gases escaped or entered by the route of the space included under ligature, it was dissipated by all the experiments mentioned in the last section; inasmuch as in the first experiment, that with the lamp-glass, the gas was seen to stud beautifully the under surface of the membrane, standing on it in minute drops or bubbles mistaken at first for water. In the experiments with baryta and lead in bags, the whole surface was covered, the precipitation taking place *only* there. Especially was it manifest in the last experiment, where the inner surface was stained black, while the solution remained clear and colourless. The gas therefore penetrates through *every part* of the membrane.

Being desirous of ascertaining more accurately the relative facilities of transmission, I solicited the assistance of my friend and pupil, Professor J. K. Finley, to whose patience, skill, and delicate manipulation, I owe much of the certainty of the following experiments.

Having constructed a syphon of glass with one limb three inches long, and the other ten or twelve inches, the open end of the short leg was enlarged and formed into the shape of a funnel, over which finally was firmly tied a piece of thin gum elastic. By inverting this syphon and pouring into its longer limb some clean mercury, a portion of common air was shut up in the short leg, and was in communication with the membrane. Over this end, in the mercurial trough, was placed the vessel containing the gas to be tried, and its velocity of penetration measured by the time occupied in elevating to a given degree the mercurial column in the other limb. Having thus compared the gases with common air, and subsequently by the same instrument, and in bottles, with each other, I was able to arrange the following gases according to their relative facility of transmission, beginning with the most powerful:—ammonia, sulphuretted hydrogen, cyanogen, carbonic acid, nitrous oxide, arsenuretted hydrogen, olefiant gas, hydrogen, oxygen, carbonic oxide, and nitrogen.

Ammonia transmitted in 1 minute as much in volume as sulphuretted hydrogen in $2\frac{1}{2}$ minutes—cyanogen, $3\frac{1}{4}$—carbonic acid, $5\frac{1}{2}$—nitrous oxide, $6\frac{1}{2}$—arsenuretted hydrogen, $27\frac{1}{2}$—olefiant gas, 28—hydrogen, $37\frac{1}{2}$—oxygen, 1 hour and 53 minutes—carbonic oxide, 2 hours and 40 minutes.

Nitrogen has a rate of penetration so low as to be difficult to ascertain, because there is no gas of a lower rate with which to compare it. Only by causing it to pass through a membrane by means of a column of mercury, is the fact of its transmission known. In that way, the quantity being compared with that of carbonic acid, its rate was found to be about three hours and a quarter.* This experiment, made but once, is not confidently relied on; but the rate of nitrogen is unquestionably less than that of carbonic oxide.

Chlorine immediately altered the texture of the membrane, as did muriatic acid gas, sulphurous acid, nitric oxide, and some others, so that it was impossible to reach, for their rate of penetration, accurate results.

In every case the movement of the gas through the membrane became progressively slower, until it totally ceased; and finally, but

* A vessel filled with atmospheric air and closed by gum elastic was submerged under water for two weeks, when it was found to contain only nitrogen gas. Possibly this arrangement may furnish a new eudiometer. It offers a new mode of obtaining nitrogen gas.

A phial containing atmospheric air, after being closed by a membrane, was placed in a receiver holding nitrous oxide. In about two weeks only nitrogen was found in the phial. These facts show the mechanically sluggish character of nitrogen *gas:* with its chemical inactivity we have been long acquainted.

more slowly, the mixed gas returned, as indicated by the descent of the column of mercury. The retrogradation ceased only when the two columns came to equilibrium, or failing the possibility of that, when the mercury in the shorter limb had reached the membrane, through which mercury has not been found able to penetrate.

Acquainted with the *fact*, and the relative *rate* of the penetrativeness of gases, the *degree of force* became the next subject of inquiry. That it was considerable, could be seen by looking at the stout membranes broken by it.

By greatly increasing the length of the taller limb of an inverted syphon, similar to the one already described, I was able to bring to bear on the common air imprisoned in the shorter limb, a very considerable column of mercury. Up to a pressure of sixty-three inches of mercury, or rather more, equal to more than the power of two atmospheres, the penetrative action was found capable of conveying the gases, the subject of the experiment, into the short leg, through the gum elastic membrane. The entrance of the gas into the short leg, was expressed by the ascent of the long column of mercury in the other, which, as it entered, it was compelled to heave up. At the height of sixty-three inches, the membrane, though supported by cloth, could scarcely sustain the weight, and would not bear any increase of height. Although, therefore, at present, I do not know the limit of this power, I believe it will be found very much greater, because the power of the column which was tried did not, until a leak was sprung, seem to very sensibly affect the rate of entrance.

To the mind of a physician, the repetition of the foregoing experiments, substituting animal membranes for gum elastic would naturally suggest itself. Should animal membranes present the same phenomena, the interest of the investigation would be vastly enhanced, and a very important service done to the cause of "Physiological Medicine." That animal membranes would act in the same manner, was rendered probable by the well-known experiments of Priestley, who affected by means of oxygen the colour of blood confined in a bladder. It had also been observed by him that a closely tied bladder, containing hydrogen gas is found after a considerable lapse of time, to contain only atmospheric air, and that, in quantity perhaps, equal to the hydrogen lost. Several other facts of the same kind are detailed by him. Finally, in the *Journal of the Royal Institution*, I find the following "Notice of the Singular Inflation of a Bladder. By Thomas Graham, A. M. F. R. S. E. Lecturer on Chemistry, Glasgow.

"In the course of an investigation of mixed gases through capillary openings, the following singular observation was made.

"A sound bladder with stop-cock, was filled about two-thirds with coal gas, and the stop-cock shut; the bladder was passed up in this flaccid state, into a bell-jar receiver, filled with carbonic acid gas over water. The bladder was thus introduced into an atmosphere of carbonic acid gas. In the course of twelve hours, instead of being in the flaccid state, in which it was left, the bladder was found distended to the utmost, and on the very point of bursting, while most of the carbonic acid gas in the receiver had disappeared. The bladder actually burst in the neck, in withdrawing it from under the receiver. It was found to contain thirty-five parts carbonic acid gas by volume in one hundred. The substance of the bladder was quite fresh to the smell, and appeared to have undergone no change. The carbonic acid gas remaining without in the bell-jar, had acquired a very little coal gas.

"The conclusion is unavoidable, that the close bladder was inflated by the insinuation of carbonic acid gas from without.

"In a second experiment, a bladder containing rather less coal gas, and similarly placed in an atmosphere of carbonic acid gas, being fully inflated in fifteen hours, was found to have acquired forty parts in one hundred of this latter gas, a small portion of coal gas left the bladder as before.

"A close bladder, half-filled with common air, was fully inflated in like manner, in the course of twenty-four hours. The entrance of carbonic acid gas into the bladder, depends, therefore, upon no peculiar property of coal gas. The bladder partially filled with coal gas, did not expand at all in the same jar containing common air or water only.

"M. Dutrochet will probably view, in these experiments, the discovery of *endosmose* acting upon aëriform matter, as he observed it to act upon bodies in the liquid state. Unaware of the speculations of that philosopher, at the time the experiments were made, I fabricated the following theory to account for them, to which I am still disposed to adhere, although it does not involve the new power.

"The jar of carbonic acid gas standing over water, the bladder was moist and we know it to be porous. Between the air in the bladder, and the carbonic acid gas without, there existed CAPILLARY CANALS through the substance of the bladder filled with water. The surface of water at the outer extremity of these canals being exposed to carbonic acid gas, a gas soluble in water, would necessarily absorb it. But the gas in solution, when permeating through a canal, it arrived at the surface of the inner extremity, would *rise* as necessarily into the air in the bladder and expand it. Nothing but the presence of carbonic acid gas within, could prevent the disengagement of that gas. The force by which water is held in minute *capillary tubes*, might retain that liquid in the pores of the bladder, and enable it to act in the transit of the gas, even after the pressure within the bladder had become considerable."

A careful perusal of Mr. Graham's notice, will excite in every one who knows the value of experimental interrogation, an expression of surprise, at the failure, on the part of that intelligent and ingenious chemist to pursue in the only true spirit of science, the investigation of a principle, one of the most striking manifestations of which had thus been placed conspicuously before him. Content with a single additional experiment, he comes, *in the ancient method*, to immediate conjectural explanation, and has thus lost an easy opportunity of making a beautiful, and perhaps extensively useful discovery. Made at an earlier period, his observation was published in the Journal for October, 1829, and has since attracted apparently no scientific attention. Such is usually the fate of the most pregnant facts which are not perceived to bear on some *generality*. This one passed from *my* mind along with all the other isolated phenomena of that number of the journal, and only shone importantly when illuminated by the reflected light of an extensive principle, subsequently developed. These remarks are made, not to throw any discredit on the character of the accomplished gentleman to whom they refer, but to correct the baneful error of ancient dogmatism, which yet weighs so heavily on the cause of nature and truth. It was true that the carbonic acid entered a closed bladder, and that too *with power*, and it was equally true, that oxygen had done the same thing in the experiment of Priestley, and that, in his hands, even common air had penetrated to replace hydrogen in a similar viscus, and yet he ascribed the phenomenon observed by him, to the *capillaries*, and the conducting power of *aqueous canals*.

In what manner the *power* of "rising into the air" was given, and whether it was dependent on the force of water, or some other cause, does not and could not be made to appear from the single fact, as presented by Mr. Graham. A very little practical interrogation, following the *word* just uttered by nature, would have obtained an answer fraught with new and important truth.

But to return to the immediate subject of this essay.—Analogy, the experiments of M. Dutrochet, and the observations of Priestley and Graham, gave me almost the certainty of finding animal membranes performing relatively to the gases the same function which belongs to those formed of the inspissated juice of the *Jatropa elastica*. Accordingly, each gas was subjected to the action of animal membranes, which replaced the gum elastic at the mouth of the short limb of an inverted syphon. Dried bladder, and gold-beater's skin, moistened to cause an approach to a normal state, and sections of various recent tissues, were successively tried, and found to act on the gases in the manner and order in which they were affected by gum elastic. The more *recent* the membrane, the more rapid and extensive the effect produced; and in *living animals* the transmission was very rapid.

Besides the estimates of comparative movement made with the syphon, experiments in a different manner were resorted to, to more clearly show the general truth. Thus a piece of the strong intestine of a goose connected with the œsophagus and gizzard, being partially inflated with common air, and firmly tied, was left in an atmosphere of carbonic acid, where *in less than ten minutes* the inflation caused it to burst. On repetition of this experiment and examination before fracture, a very large quantity of carbonic acid gas was discovered to have entered the intestine. Crop, bladder, &c. &c. of recently killed animals produced exactly similar results. Perhaps the following experiment will be esteemed even more satisfactory. Carefully removed from the chest of a snapper, *(Testudo serpentaria,)* its lung was partially inflated with common air, and confined there by a ligature on the tracheal tube. Exposed in this state, to an atmosphere of carbonic acid, or nitrous oxide, it became very soon fully inflated by the gas, to which exposed, as subsequently proved by chemical examination. Less than half an hour of exposure sufficed for the full inflation of the lung, which was removed only when it threatened to burst. Containing a portion of nitrogen, it was left exposed all night to an atmosphere of oxygen, yet scarcely enough entered to signify its presence; in quantity superior to that which is held in atmospheric air. A taper appeared in it somewhat brighter than before its immersion.

In a subsequent experiment, the two lungs of a snapper having been extracted, were inflated respectively, with common air, and carbonic acid gas. So prepared, each lung was surrounded by a bell-glass, containing an atmosphere of the other gas, so that common air surrounded the carbonic acid *et vice versa.* That lung which contained common air, soon burst by the infiltration of carbonic acid, while the other collapsed by its escape.

In concluding the series of experiments, on *the question of fact*, some were made on *living animals.* A quantity of solution of acetate of lead having been thrown into the peritoneal cavity of a young cat, sulphuretted hydrogen was discharged from the pipe of the generating retort, directly into the rectum. In four minutes the poisonous gas killed the animal, giving to it, because of enormously dilated pupils, a very wild aspect. Instantly on its death, which was itself an affair of a moment, the peritoneal coat of the intestines, and the walls of the cavity in contact with them, were found lined with a metallic-looking precipitate, adherent to the surface, and susceptible of removal by nitric acid, moderately diluted. It was the characteristic precipitate of sulphuretted hydrogen when acting on lead. When in another experiment, the abdominal cavity was almost instantly opened, only the intestines and stomach presented the bronzed aspect; the peritoneum of other parts, and the bladder, appeared of their natural colour, thus proving, that the gas had infiltrated, and not passed through any rent or fracture, an event which would have stained the whole of the lining membrane of the cavity, and dyed the bladder. This experiment forcibly reminded us of that where the internal surface of a gum elastic bag holding lead water, was stained black by sulphuretted hydrogen, while the solution continued pellucid.

In another experiment on a cat, a solution of acetate of lead was placed in the thorax, and sulphuretted hydrogen in the abdomen. Almost immediately, on the entrance of the sulphuretted hydrogen into the abdominal cavity, death ensued, with the same dilatation of pupil as before. On inspecting the thoracic side of the diaphragm, which was done as quickly as possible, the tendinous part of it displayed the leaden aspect of the precipitate by sulphuretted hydrogen. Many years ago, in 1823, while engaged in investigating MAGENDIE's theory of venous absorption, I coloured the diaphragm of a living cat blue, by placing a solution of prussiate of potash on one side, and that of sulphate of iron on the other. At that time I supposed the effect to be *vascular*, but the experiments on membranes of gum elastic, afford an explanation which more rationally refers it to *organic molecular infiltration;* for, in such membranes, vessels cannot possibly exist at all; and as animal membranes act in a manner so perfectly accordant with that of the coagulated vegetable juice, it would be judging against evidence, to refer their agency to widely different causes. At the same relative rates, with the same power, and that a great one, they could scarcely act, in obedience to causes so dissimilar as those alluded to.

Every one who has read the beautiful memoir of Dutrochet, on "*L'agent Immediat du Mouvement Vital, &c.*" and who has, as nearly all have, suffered their belief to be swayed by his eloquence of fact, method and style, will on a cursory glance at the experiments detailed in *this* paper, refer them to the "NEW POWER" so ably contended for by the French naturalist. That they depend on the *same power* cannot be reasonably questioned, whether that power be one long known, or recently discovered. In his experiments made exclusively on liquids, and developed with surpassing good fortune and sagacity, he proved the transmission of liquids through animal membranes, and saw them penetrating too *at different rates*, some solutions passing rapidly, some with greater slowness, some in scarcely appreciable quantity, and some never passing at all. Their *force* too, he found to be of estimable amount. In fact, every aspect of the two sets of experiments, tends more and more clearly to induce a reference of them to one and the same cause, whatever that cause may be. Although the facts presented by him demonstrate all this, yet M. Dutrochet did not perceive it, as is evident, from his reference of the phenomena to a source to which, in latter years, the French naturalists and philosophers have been accustomed to look with almost superstitious reverence. Electricity is the great key of scientific explanation; and the theory of Du Fay is relied on, though badly itself sustained, as the *point d'appui*, of almost all other theories. M. Dutrochet has accordingly ascribed the transmissions to that power, and supposed, in the very teeth of some of his most striking facts, that the current was from a less dense to a more dense fluid; or from positive to negative, dependent not on an inherent power of infiltration, and of course for the same membrane always the same, but varied or even inverted at pleasure, by arrangements productive of supposed electrical powers. He says, p. 129.

"Ces resultats nous font déjà pressentir que l'impulsion qu' éprouvent les liquides dans ces expériences, dépend d'un courant électrique déterminé par le voisinage de deux fluides de densité ou de nature chimique différentes, fluides que sépare imparfaitement une membrane perméable. *Cette membrane ne joue évidemment aucun rôle propre dans cette circonstance; elle ne fait fonction que de moyen de séparation entre les deux fluides* auxquels elle est cependant perméable: *les liquides la traversent, soit dans un sens, soit dans l'autre, au gré de l'action réciproque des deux fluides qui baignent ses parois opposées.*"

As he used water and solutions in water, by which the former became denser, he found, as might be expected, that it infiltrated the tissue more readily than most of its solutions: hence, in such cases, the water penetrated more quickly than they, and the current usually set most rapidly from less dense to more dense. But when he used essentially different liquids, he yet found the water going through at *its high rate*, as we perceived to be the case with sulphuretted hydrogen and ammonia. Water traversed the animal membrane rapidly, to join *alcohol*, which, according to his electrical theory, should not have been the case, as the alcohol is less dense than water. For this and some other exceptions, Dutrochet attempts to account, by reference to influence, derived from *chemical qualities*.

If, however, as in the case of the gases, two *liquids* of different rates of penetrativeness, be placed on opposite sides of an animal membrane, they will in time present the greater accumulation on the side of the less penetrant liquid, *whether more or less dense*, but will finally thoroughly and uniformly mix on both sides, and at length if any pressure exist on either side, yield to that and pass to the other side.

As some substances have no penetrativeness, such as milk or blood, or at least their solid parts, the water placed on the opposite side of the membrane alone moves, and it is only after the decomposition by putrefaction, and consequent formation of a *new fluid having penetrant properties*, that any current sets in the direction opposite to that of the water. To prove this, it is only necessary to show that *alcohol* penetrates gum elastic much more rapidly than *water;* and that therefore when that kind of membrane is interposed between them, the greater current is from alcohol to water and not from water to alcohol.

A hollow glass cylinder, open at both ends, was closed completely by two membranes of gum elastic having been previously perfectly filled with alcohol. It was then sunk in the large pneumatic trough of my laboratory, where it remained one week. At that time it presented a concavity at each end, of decided depth, proving the escape of a considerable quantity of alcohol. On the other hand a similarly prepared vessel filled with water and submerged in alcohol, presented at the end of a week well marked *convexities*, demonstrating the insinuation of alcohol. If it be contended that the nature of the membrane affects and even reverses the electrical state, it may be well said in reply, that there is no analogy for that, and moreover the same membrane acts under the movement of *gases* precisely as an animal membrane. The supposition would invest it with a most Protean character.

In making experiments for the preparation of gum elastic by ether, that liquid was found to readily infiltrate its tissue. Alcohol has been already shown to penetrate it better than water, and water enters its substance so slowly, that a bag of a thinness productive of almost perfect transparency, and containing four ounces, two drachms, fifty-seven grains, lost by evaporation but eight grains in the first period of twenty-four hours, and fifteen grains during the next three days. Viewing these facts, a prediction was founded on them relative to the effect of placing ether in contact with one surface of such a membrane, while alcohol or water, occupied the opposite surface. As was expected, the greater quantity accumulated on the side of the less penetrative substance, and the ether always caused by its transmission an augmentation of liquid on the side of the alcohol or water. Using *animal membranes*, facts of a similar kind, previously ascertained, led us to anticipate the *opposite* result. According to expectation, water being most penetrative, passed through so much more rapidly than ether or alcohol as to swell the amount of liquid on their side.

When alcohol is largely diluted with water, it penetrates an animal membrane more easily itself, and offers to the pure water which reaches it from the opposite side less invitation to infiltrate it, according to a law of *progressive diminution*, pointed out by our experiments on gases. Such a diluted portion of alcohol placed by M. Dutrochet in his endosmometer, and raised above the level of the pure water on its outside, found, in the force of the higher column, sufficient cause for its escape, which continued until the level was reached, when action apparently ceased. If the level be obtained at the commencement of the experiment, either no appreciable change is observed, or the movement is unquestionably in a direction contrary to that stated by Dutrochet. So, when gases are permeating in opposite directions any interposed membrane, the penetration soon begins to lessen, because there is on either side less porosity unoccupied, and there is also in them the repellent character of their gaseous state. M. Dutrochet reconciles these apparently contradictory facts to his system, by supposing chemical influence to produce the first, and electricity the second. In either case he does not appear to dream of independent and original powers of penetration, by which the liquid comes through to the opposite side of the membrane, *remaining* in *its* tissue, or passing on by a similar power of infiltration into new matter, or such matter being absent, accumulating on that side by the influence of mechanical power, or electrical excitement, or chemical combination, truths adequately demonstrated by my experiments on gases.

The blinding effect of preconception on the most philosophic and candid mind, can perhaps have no better exemplification than is afforded by what M. Dutrochet says relative to the point of accumulation, when a *diluted acid* and water were placed on opposite sides of an animal membrane. As alkalies produced towards them a current, for the support of his electrical theory, acids should be found to set the current towards water, and *he found it so.* In my experiments, the greater current was *always* towards the acid and not from it; and I find that Dr. WEDEMEYER, (Untersuchungen über der Kreislauf des Bluts,* &c.) has made the experiment with a like result. On reference to Dr. TOGNO's experiments, (Amer. Journ. of Med.

* See this Journal, Vol. V. p. 199.

Sci.) which were chiefly repetitions of those of Dutrochet, we perceive that he does not seem to be satisfied perfectly with the report of Dutrochet on *this* subject. Let any one desirous of testing this matter, tie a piece of animal membrane over the end of a hollow glass cylinder, partially fill it with diluted sulphuric acid, and place it in a vessel of clean water, so as to bring the two columns to a level. In a few hours the column holding the acid, will rise considerably above that of the clean water, proving the greater current to set from water to acid, and not from acid to water. Tests, however, show that *some* acid does pass the membrane.*

To feel *assured* of the error of Dutrochet, I repeated the experiment in another form. A tube of five-sixteenths of an inch in diameter, ending in a funnel-like extremity of an inch and a quarter, was covered at its broad end by animal membrane, then partially filled with diluted acid, and placed, membrane downwards, in clean water, so as to bring both columns to a level. INSTANTLY the rise in the narrow tube was perceptible, and amounted to nearly half an inch in half an hour. Reversing the order by placing the clean water in the tube, and the diluted acid without, as sudden and progressive a descent of the column of clean water was observable. Tests, after a short time, betrayed the percolation of some acid, and finally, in every case the liquid became uniformly acidulous throughout, and the two columns fell to a common level, an event which may always be expected, unless the combination produced by transmission is not penetrant.

Water may be removed from the surface of a membrane at which it has arrived, in many and various methods. Invitation may be given to it by a column of mercury, contained in a hollow cylinder closed above by animal membrane. Water readily passes through, may be seen studding in drops the surface of mercury, gradually covering the under side of the membrane, causing at length the separation and descent of the mercury, and continuing to enter the cylinder, until the mercurial column sinks to the level of the general contents of the trough. There the action ceases, but if the water placed *above* the membrane, be now removed, the mercurial column will again rise, and all the water having escaped through the membrane by the process of infiltration into the atmosphere, the mercury will be finally seen in close contact with the membrane from which it had receded. Sometimes before the completion of the process, a change takes place

* This fact I demonstrated to Dr. Togno.

in the condition of the animal matter, and some gas being introduced below, suspends the ascent of the mercury.*

A sponge *slightly* moistened, or dry oat meal, or any other absorbent, placed by means of a moderate weight, closely in contact with the membrane, will, by absorbing the water, cause its continued permeation.

Even *vis a tergo*, as in the instance of the gases, will produce infiltration where there exists no other cause of penetration. Over the end of the short limb of an inverted syphon was tied a piece of bladder, and over that, and *in close contact with it*, was also secured a piece of sheet caoutchouc. Water was then placed in the short limb, in communication with the bladder, and thus left for a few hours without compression. No appreciable amount of infiltration ensued. But, in a short time after a column of mercury had been placed in the long limb, water was plainly seen to insinuate itself through the bladder, and to raise up and separate from it the more elastic membrane which surmounted it. After all the water had passed into the space between the two membranes, the syphon was placed in its ordinary position, the end of the long limb resting in the mercury of the trough. Soon, the water repassed the bladder, ascended through the short column of mercury lying above it, and collected in the curve which then formed the pinnacle of the apparatus.

Another fact, in itself important, bears forcibly against the electrical theory of Dutrochet. To try the absorbent power of the dermoid tissue, pieces of it in a recent state, were tied, cuticle outwards, over bottles which contained common air or carbonic acid gas. Over the bottle which held carbonic acid, was inverted a jar of common air, and over that holding air was placed a jar of carbonic acid. The more penetrating gas was in the one case in contact with the cuticle, and in the other with the dissected under surface of the skin. A trial of the contents, after twenty-four hours, showed that much more carbonic acid had penetrated in that apparatus where it was applied to the cuticle, than in the other. As in that case it had gone from the jar into the bottle of common air, while in the other case very little carbonic acid gas had escaped from its receptacle, I filled it again, and tied over it a piece of skin with its cuticle looking inwards. In twenty-four hours the carbonic acid was equally diffused through both bottle and jar. Two similar sections of intestine were slightly inflated with common air,

* A new hygrometer was suggested by this experiment, of which I purpose giving an account to the Philosophical Society.

one of them being turned inside out. Both having been carefully tied at the ends, were placed in identically the same carbonic acid, in vessels of equal size. It was soon apparent that the one which had been inverted, filled itself most rapidly, and although rather less than the other, soon greatly exceeded it in size and hardness. After remaining so exposed for eighteen hours, vessels of common air were placed over the distended bags, when a diminution of volume became in time apparent, and was more rapid considerably in the specimen which had not been inverted. It appears then, that the transmission of a gas is easiest where it is placed on the cuticular or mucous surface of an animal membrane, rather than on its cellular or peritoneal surface, a fact to be kept in view in rating the transmissibility of the different gases or liquids. The fluids should be compared under exactly similar circumstances, standing in the same relation to the surfaces of the membrane used.

In the following experiment, made with great precaution, we perceive a result distinctly indicative of the superior penetrability of the cuticular surface. Over the mouths of two phials, accurately filled with alcohol, weighing according to a *Pese-Ether*, thirty-five and a quarter, were tied two pieces of human skin. In one the raw side presented, in the other the cuticular side. Both were placed mouth downwards in similar specimens of water, with columns of equal altitude. After the lapse of twenty-four hours, the alcohol was examined, and found to weigh more, by at least one degree, in the phial which presented the cuticle to the water. In it the etherometer sunk to thirty-three and a half, while in that which presented the dissected surface to the water it fell only to thirty-four and a half. The one had been reduced by the water one degree and three-fourths, and the other only three-fourths of a degree.

In all these cutaneous experiments, we perceive not only the agency of the membrane itself, but even that of its respective surfaces, so that we are not at liberty to admit the assertion respecting the action of the liquids, as independent of the influence of the intervening membrane.

In truth it is now manifest that the liquid, if penetrative, permeates a given tissue at a rate dependent on the character of tissue and power of penetration. If on the opposite side there exist a substance or power capable of occupying or removing it as fast as, or faster than the membrane delivers it, the actual rate of transmission will be as high as is possible; but if not so capable the accumulation will be at a lesser rate, and will represent the degree of permeability of the inviting substance alone. Thus, for illustration, if ether can convey away water as fast as, or faster than, the membrane can transmit it, the rate of penetration will be the greatest possible, and will represent the full penetrability of that membrane by water. But if ether is less penetrable than membrane, the rate of accumulation will not represent the power of the animal tissue, but that of the ethereal interstices, which, on the supposition, is less.

The *power* of this process in liquids, like that of the gases, is not *yet* measured. It is the *power of infiltration* in all such cases, and must be eminently great. Like all processes having dependence on molecular action, this one is influenced by electricity, when that is brought to bear on it, but we can scarcely, after a fair estimate of the value of facts, see any thing more in the power than that of common interstitial infiltration, a power marvellously great, but insusceptible of demonstrative reference at present to any known cause.

The amount of force having been shown to greatly exceed that of atmospheric pressure, we feel assured that the interstices are penetrated not by any *vis a tergo*. It must therefore be attributed to some species of *attraction*, the force of which, as shown by the condensation of some gases by charcoal, sometimes equals a power of forty atmospheres, or nearly six hundred pounds on the square inch, a power amounting nearly to that of steam, *at its maximum density*.* It is not chemical, because the quantity absorbed bears no relation to known affinities; it is not homogeneous attraction, for it takes place *solely* among dissimilar substances, and often subverts the condition produced by that power as in some cases of solution.

After having proceeded thus far with my argument and experiments, I felt as if it were important, if not essential to my positions, to test the power of gum elastic as an absorber of gases independently of the artificial arrangements which brought different gases to the opposite sides of it. For that purpose I selected a hollow cylinder of gum elastic, with thick parietes about an inch in length. This specimen was placed in a cylindrical graduated test-glass, filled with carbonic acid gas and placed over mercury. In less than one minute the mercury began to rise, and in eight hours, during which the observer was absent, it had risen to a considerable height. A rough attempt to measure the bulk of mercury raised, and of gum elastic used, showed that nearly an equal volume of carbonic acid had been absorbed by the caoutchouc. A piece of dry bladder was subjected to the same treatment, and produced a similar rise of the column of mer-

* Found by comparing the experiments of Cagnard de la Tour with those of the Committee of the Institute of France.

cury. Macerated in water for an hour, and then wiped well with a dry towel, so as to obtain dry surfaces, the same piece of bladder was again placed in the gas over mercury, and produced a diminution apparently equal in quantity to that which, when dry, it occasioned.

The bulk of the gum elastic was considerably increased by the infiltration, so that although easily placed in the glass vessel, it was of difficult removal. This fact, added to that of the thorough penetration by water of an animal membrane macerated in it, shows how much of the phenomena described in this paper is attributable to the organic molecular infiltration. The remainder of the effect is dependant on the moleculo-porous relation of the gas or liquid to the substance beyond, into which infiltration carries the permeating substance. If the recipient beyond the membrane be as active as the membrane, or more so, all that the membrane brings to its surface will be transmitted as fast as it arrives; but if that recipient be of inferior penetrability, less will pass on than the membrane could carry through, and in that case the rate of penetrativeness of the substance relative to the membrane is inappreciable. Any gas penetrates another gas better than it does any solid, hence we obtain for *them* the true rate. But liquids penetrate each other sometimes less rapidly than at the rate of the transmission through the membrane. Such cases do not show the rate of transmissibility by the membrane, but of reception beyond.

Having completed the *first* series of experiments on molecular infiltration, before entering upon an account of the second, reserved for the next number of the Journal, it may be refreshing both to experimenter and reader, in a very toilsome investigation, to pass in cursory review, some of the almost infinite theoretic and practical suggestions, which flow from the facts before us.

The most striking generality, is that of the *high power* of penetrativeness of *gases* for *organic molecular tissue*, long known to be infiltrable by liquids, but until now, not generally known to admit of any permeation, by at least, *insoluble* aëriform substances.

Secondly. We are struck with an unexpected result, the *great* POWER of gases to infiltrate *each other*. It has been long known, that aëriform substances confined in the same apartment, finally mingle uniformly, and that, even if the lighter one be placed above the other. To account for this, and some other facts of the same class, Mr. DALTON supposed that each gas, in reference to the vertical relation of its particles, stood in an attitude of independence of any other gas present, as much as if no such gas were confined along with it, no particle of one gas being supposed to rest on any particle of the other, the interstitial cavities of one gas being in fact a *vacuum* for the reception of the molecules of the other, each for each.

The *power*, however, of this infiltration being known, we are entitled to conclude, that the interspaces of gases are reciprocally occupied with a force similar, and probably equal to that which causes the imbibition of liquids by solids, and produces solutions of substances, even of the highest cohesive attraction. Solutions may now be esteemed infiltrations by solids and liquids of the tissues of each other, *requiring*, perhaps, only a *fitness in size*, rather than a chemical or cohesive attraction, for we see it subverting even the greatest cohesive power, and holding no apparent relation with known chemical affinities.

The atmosphere cannot any longer be considered as a mixture in the common acceptation of the term. Its gases penetrate each other interstitially with great mechanical force, so great as to defy all mere mechanical means of separation. It is an exemplification of solution.

When the particles of a solid separate and enter the tissue of a liquid, it is termed solution, when the liquid penetrates the solid, and the latter maintains its solidness, it is usually called infiltration, imbibition, absorption, &c. &c. The processes are perfectly alike in principle, the different names being expressive of that, and of certain accompaniments or effects also.

By means of our second generality, we are enabled satisfactorily to explain many phenomena not heretofore easily accounted for. Thus we understand how a gas or odour flows so rapidly through the whole tissue of a still atmosphere, and why some gases do so more speedily than others. An explanation is also given of the diffusion of odours, even against a draught, or current, and it accounts for this fact, among others, that brimstone thrown into the fire, is perceived by the olfactories, when the draught of the chimney is even perfect.

As proved in some experiments, already detailed, many solids are dependent on water for the power of penetrating tissues, or gases, &c. and it appears probable that many odorous solids, in particular, enter the atmosphere, solely by penetrating its hygrometric constituent. Thus, in solution, colouring matters readily in certain cases, pass through membranes impenetrable without such aid, and every one has perceived the singular smell of a dusty road, after a shower, even at a very considerable distance. In a damp day, or immediately after rain, we more distinctly and vividly enjoy the fragrance of the parterre. Malaria seems to be dependent on the same cause for its penetration into the atmosphere, for every one knows the greater hazard of a residence in low damp situations, and the general unhealthiness of a damp summer, or autumn. As electricity is a great hydragogue, and substances in a negative state forcibly attract moisture, we might expect to find that season most damp and unwholesome in which the atmosphere maintained an electro-negative condition, and that driest and most healthful when it was electro-positive. Facts on this subject are yet to be created; but this one presents an aspect german to the subject. Mr. William Mason, of Philadelphia, a philosophical instrument maker, respected both for his ingenuity and correct moral character, informed me, that when, in 1820, the yellow fever existed here as an epidemic, he could not excite an electrical machine at his residence, in the infected district, although at his shop, which lay at some distance from it, the operation of the machine was sufficiently powerful.*

There exists between the lower surface of air, and the upper surface of water, a space possessed of powers analogous to those of the interspaces of substances in general. Along this plane, certain substances dart with surprising facility, losing as their particles separate, all cohesion, and acting repulsively. The oils are remarkable in this regard, and camphor exhibits, because of it, curious and agreeable movements, when thrown upon perfectly clean water.†

* Aqueous gas penetrates the air more or less rapidly according to the temperature and moisture of the atmosphere. According to our law of *progressive diminution*, evaporation is slower in a moister atmosphere and *vice versa*. The following experiment shows that aqueous gas has also *its rate* of penetration. A long tube, surmounted by bladder, held water and mercury; the former of which being above, was in contact with the membrane. Although the mercury rose gradually as water escaped, yet some air found its way through the bladder, and occupying the upper part of the tube, separated the liquid and bladder from each other. Under such circumstances only, air and aqueous gas could reach its lower surface. Notwithstanding this, and the gradual increase of the quantity of air, the mercurial column continued to rise, showing that the rate of the penetration of aqueous gas, is greater than that of atmospheric air, by which it could not be counterbalanced. Curious to see the effect, I tied over the summit of the tube, a bag, holding carbonic acid, which thus replaced the atmosphere. Almost immediately, the mercury gave intimation of descent, by losing its convex summit. It did fall, and carbonic acid entered through the membrane, faster than the moisture had at any time escaped.

† The best mode of examining this property of camphor is the following:—Take a piece of cork, a flat four-sided prism, and attach to its narrow sides, close to the ends, and diagonally opposite to each other, two small pieces of

But it is chiefly with reference to physiology, pathology, and practical medicine, that we see in the foregoing experiments, things of much real value. They throw a particular light on the functions of respiration and cuticular absorption, and will probably lead to the employment of gaseous agents of cure with confidence and certainty.

The experiments on the mutual action of gases and liquids, show that although a gas may, when *alone* presented to a liquid, for which it has no chemical affinity, penetrate its molecular cavities, yet, it will again leave it to join any gas whatever, which is brought into communication with the liquid. Thus carbonic acid or nitrous oxide readily penetrates blood or water, but returns from either into the air or any other gaseous substance, which contains no carbonic acid, or nitrous oxide. It is in this way, probably, that the oxygen disappears, and an exactly equal quantity of carbonic acid replaces it in the bronchial cells. Oxygen penetrates slowly the membranous tissue, to infiltrate and brighten the blood; carbonic acid is immediately formed, and being a gas differing from the remainder of the air yet in the air-cells, its tendency is to return, to penetrate that air, and thus escapes through the trachea along with it. The oxygen enters, because there is oxygen enough behind to permit that, and it is also an observed fact. The carbonic acid formed, makes its escape, because invited by the molecular tissue of atmospheric air. Keeping up any reference to known facts, we can scarcely doubt the truth of our explanation, or venture to adopt any other. The investigations of John Davy, and our careful repetition of his experiments, with others, fully as conclusive, leave no doubt of the entire absence of carbonic acid in the blood.*

camphor. Resting with its broad surface upon a considerable plane of *quite clean water*, the apparatus will regularly rotate, and that either until the camphor is consumed, or the interspace is filled with that substance, or an emanation from it. Oil, by filling the space, immediately suspends the motion. If a cork be greased slightly, or camphorated at the end, it will move in a direction from that end, and with considerable velocity. The same thing happens when fine dry flour is attached, or when the but end of the cork is dipped into ether or alcohol. A cavity being made in the upper surface of a floating cork, near the end, filled with ether, and connected by a cotton filament with the water, it will sail about a pneumatic trough for a considerable time, always moving towards the solid end. A little rudder being attached to the cork, and slightly inflected, the vessel may be made to sail entirely round a circular tub.

* Having filled a phial with hydrogen gas, blood was received into it from a vein, so as to exclude the agency of oxygen. When completely full of blood, the phial was closed by sheet gum elastic, and immediately subjected to the

It must, therefore, be produced in one of two modes, either by the penetration of oxygen into the blood, and its union there with carbon, or the exit of carbon from the blood, to unite with oxygen in the air-cells. Now, as carbon is one of the most fixed substances in nature, and has not been proved capable of such transmission, we are, if *facts* be our guide, compelled to adopt the other theory, which is perfectly in accordance with the laws of gaseous infiltration If it be asked how the carbonic acid is formed in the blood, at so low a temperature, we reply, that carbonic acid is actually created *at a lower temperature*, by the agency of infiltration, when oxygen gas is imbibed by a piece of fresh cold charcoal. The difference in the rate of permeation, is quite sufficient to account for the escape of all the carbonic acid formed by the infiltrated oxygen.

Our theory does not account for the production of animal heat, but it is presumed that no well-informed physiologist now seeks for it in the action of the lungs, or the process of decarbonization. The simple fact, that cold-blooded animals breathe without *any* increase of temperature, proves that mere breathing to *any amount* will not produce heat. Like all the other animal functions, that productive of heat is dependent on a normal condition of blood, and is thus *indirectly* governed by the act of respiration. As in cold-blooded animals, there is no apparatus for producing heat, respiration does not in any way influence *their* temperature. So in some of the cases quoted by JOHN HUNTER, where blue-boys maintained a temperature preternaturally great, the blood was very imperfectly decarbonized. In such cases the calorific function found some novel stimulant.

Our experiments afford ready explanations of the effect of the various gases when respired. Carbonic acid not only cuts off the necessary supply of oxygen, but also penetrates into the blood, and passes through the route of the circulation.

We perceive why nitrous oxide, so identical with oxygen in all its chemical habitudes, should act so differently on the human system. It penetrates at least sixteen times as rapidly, and probably acts then solely as oxygen would do. Hence we see why it does not exhaust us; for it not only acts upon excitability, but creates a fresh supply of it, so that its consumption is not felt. We can also easily see why an animal was destroyed in ten minutes by breathing hydro-

action of the air-pump. Under such circumstances, no gas of any kind could be immediately separated from the blood: but after coagulation was completed, a bubble of air, about the size of a pin's head, was perceived beneath the membrane, and that that was atmospheric air, or nitrogen, was proved by its long continuance there, without apparent diminution or escape.

gen, while carbonic acid produced the same effect in two minutes. In Section I. Article IX. of his Physiological Researches, BICHAT relates some curious exemplifications of the passage of gases into the blood-vessels through the lungs of living animals. For instance, hydrogen gas could be set on fire, as in bubbles it escaped from a remotely situated *blood-vessel.* As he had used some force by means of a stop-cock and syringe adapted to the trachea, to throw in, and retain the gas, he ascribes its entrance to that cause. We see however that though impulsion augments the effect, yet that it is existent independently of any *vis a tergo.* Gases not *at all* soluble in blood, will not pass without force, but that force is, in some degree, applied in every act of expiration. Those soluble in blood, find ready entrance when not held back by the interstitial molecular power of the other gases with which they enter the bronchiæ.

The emptiness of the blood-vessels after death, or rather their fulness of gaseous matter, is no longer a case of difficult solution. Always present in the air-cells after death, air and carbonic acid gas must find a ready entrance into the emptied capillaries of the lungs, always prompt to dilate through the influence of the elastic matter which exists in and around them in the lungs. As any kind of air acts as a stimulant to the heart's cavities,* a gaseous circulation is kept up, and the aëriform matter passes into the great channels of circulation.

It does not appear difficult to understand why so penetrating and poisonous a gas as sulphuretted hydrogen should often exist in the intestines without injury; for, being mixed up with other gases, its tendency to infiltration is greatly restrained. When undiluted, its diffusion through the whole system is fearfully rapid.

"Of all the gases," says Dr. URE, "sulphuretted hydrogen is the most deleterious to animal life. A greenfinch plunged into air which contains only $\frac{1}{1500}$ of its volume, perishes instantly. A dog of mid-

* In 1823 being engaged in dissecting a sturgeon, (*Acipenser brevirostrum?*) its heart was taken out and laid on the ground, and after a time, having ceased to beat, was inflated by mouth for the purpose of drying it. Hung up in this state it began again to move, and continued for ten hours to pulsate regularly, though more and more slowly. Left at 1 A. M. in slow motion, it was found next morning still and hard. When last observed in motion, the auricles had become so dry as to rustle as they contracted and dilated.

With the heart of a *Testudo serpentaria,* (Snapper,) I lately repeated the experiment, and found it beat well under the influence of oxygen, hydrogen, carbonic acid, and nitrogen, successively thrown into it. Water also stimulated it perhaps more strongly, but made its substance look pale and hydropic, and in *one minute* destroyed action beyond all known means of restoration.

dle size is destroyed in air that contains $\frac{1}{800}$, and a horse would fall a victim to an atmosphere containing $\frac{1}{250}$.

"Dr. Chaussier proves, that to kill an animal it is sufficient to make the sulphuretted hydrogen gas act on the surface of its body, *when it is absorbed by the inhalents.*"

One of the objections to the belief in aërial poisons most confidently urged by *antimiasmatists*, is the absence of all proof of absorption of gaseous matter, and indeed this was the sole difficulty of any real moment in the way of the triumphant establishment of the theory of miasm. Will it now be going too far to say, that this difficulty is removed, and that we can explain why miasmata affect persons so differently who reside in different apartments of the same house, or who lived on opposite sides of the same street. Although being a very little nearer to the source or to the ground may not appear important, yet the difference of a few yards makes in either case a momentous distinction. Very near to its source a gaseous substance occupies a larger portion of the atmospheric space, and presents not only more matter, but matter less restrained by the molecular power of the air with which it is mingled. Not only is a greater quantity presented, but it is withheld from admission into the tissues by a slighter restraint.

As pressure unquestionably affects the rate of gaseous infiltration, *a difference in the amount of atmospheric pressure* will perhaps be considered of some importance, and *assist* in accounting for the general unhealthiness of low situations, and intertropical latitudes.

Spontaneous evaporation has been long a subject of interest to the philosopher, and has not hitherto admitted of adequate explanation. Now we perceive, that in elevating moisture into the atmosphere, a very powerful agent is at work, one capable of subverting the cohesion even of solids, and of producing the continued infiltration of the atmosphere. Heat being also capable of destroying the attraction of aggregation, augments evaporation and interstitial infiltration. On this, (I speak it hesitatingly,) depends the power of steam. Caloric penetrates gases as they do each other, and escapes from them in exactly the same manner when substances which contain less of it invite its penetrant power in a new direction. Thus, for illustration, carbonic acid penetrates common air, and, so far as we know, will expand it, if constantly supplied, to an amount of power not yet measured. But so soon as another gas or penetrable substance is presented, it begins to withdraw from the air and to penetrate that. The hollow intestine used in one of our experiments was powerfully inflated by its entrance, and yet as rapidly collapsed when the gas was invited outwards by the presence of another gas on its exterior. The resemblance of phenomena does not end here. Each penetrates different substances with different degrees of facility, and the *quality of the surface* is often to both as influential as the character of the substance which affords it. The *fact*, the *force*, the *enlargement of bulk*, the *penetrativeness varying usually with the substance and surface to be acted on, being however uniform relative to all gases*, the *constantly diminishing rate of progression*, the *issuing out again when invited by new substances, or a vacuum, or when mechanical compression is applied*, all afford evidence of analogy as perfect as is perhaps ever offered to the view of philosophy.

We are struck with its resemblance *to water* in one respect. Highly concentrated caloric invites the penetration of all liquids, and perhaps of all solids, and thus, while held in solution by it, they obtain a penetrativeness themselves which does not naturally belong to them, and are elevated into the atmosphere in spite of specific gravity however high, or of atomic weight however considerable. Some facts not yet sufficiently studied, lead me to the perhaps hasty conjecture, that even the *decomposing* influence of caloric is owing to this power. Water exercises it in that way in some cases, such as that of acetate of lead.

The great length to which my remarks have unexpectedly extended, and the call of the printer, prevent me from going fully into the consideration of the connexion of our experiments with pathology and therapeutics. Their bearing on these departments of medical science will furnish subject matter for a future essay. In the mean time, we feel entitled to believe that we better comprehend some of the phenomena of colic, tympanitis, and emphysema, and see more clearly the cause of the value of certain methods of cure.

Bichat was among the first to produce the passage of air of various kinds into the blood-vessels and cellular tissue of the lungs, by forcing it into the air-cells and there confining it. Even when the blood-vessels were full of froth, and emphysema became extensive, he could perceive not the slightest laceration of the bronchiæ. When the impulsion was moderate, the air passed only into the blood-vessels; when more violent, its presence became manifest in the cellular tissue. In certain cases referred to by authors, violent exertion, laborious respiration, and severe flatulency of the intestines, have forced air into the blood-vessels and cellular tissue. Colic has produced also tympanitis, and few practiced physicians are ignorant of the fact, that great gaseous distention of the abdomen has disappeared without the *apparent* escape of any wind. When we consider attentively the laws by which are regulated the entrance and exit of gases under the action of their penetrativeness, we feel scarcely at a loss to understand these phenomena.

The prodigious accumulation of gas in the stomach and bowels, in hysteria and epilepsy, may be explained, by supposing the air, which exists by infiltration in every part of the animal economy, to be forced by the violent compression of spasmodic action into the hollow viscera, where already existent gases invite its entrance. In some experiments on the effect of certain gases on living cavities, made by my ingenious friend Dr. Finley, their escape was so rapid as to create surprize.*

The establishment of the fact of the penetration of liquids, each according to its peculiar rate, and the modifications of that rate dependent on extrinsic force, such as impulsion or invitation, electricity, &c. teach us many valuable lessons both in philosophy and medicine. Especially I would invite attention to the cause of the remedial influence of *pressure*, as auxiliary to other means of cure.

Recapitulation.—1st. Substances formed of organic matter are generally penetrable by gases of all kinds, and by several, if not by all liquids.

2d. Each animal or vegetable tissue is differently penetrable as to time by different fluids.

3d. But all fluids penetrate any particular substance at rates susceptible of being ascertained. The gases retain the relation observed by reference to one substance in all other cases. Whatever may be the greater or less penetrability of any given tissue, the gases penetrate it, relatively to each other, according to the ratio observed in experiments on other tissues.

4th. The *ratio* is not so uniform in the instance of denser fluids. Liquids, though rateable with regard to permeation of any given substance, do not act similarly on different organic substances. Thus water penetrates most, if not all animal tissues, better than any other liquid whatever, and consequently passes through them to accumulate in *any of its own solutions*, and in alcohol or ether; while these two latter substances penetrate gum elastic with more facility than either water or its solutions. Therefore, with regard to gases, the *ratio* of penetration depends on them alone, while, in the case of liquids, it depends on the joint agency of both liquids and tissues.

5th. When the quantity of the fluids is limited, there is a gradu-

* North Amer. Med. and Surg. Journ. No. VI. 1827.

ally diminished rate of progression as the infiltration proceeds. It is proportional to the state of dilution, and ceases when the substances have become, on both sides of the membrane, of uniform condition, unless some extrinsic power is then operative.

6th. The power of the penetrativeness is very considerable, being *certainly* superior to that of two, and *possibly* equal to more than that of forty atmospheres.

7th. Penetrativeness acts not only on organic tissues, but also on gases and liquids, and with apparently equal power on all. For, after permeating a membrane, the gas or liquid goes on into the molecular tissue of the gas or liquid beyond, and no pressure which the membrane can bear, acts as a restraint on the progression.

8th. Although of such high mechanical power, the penetration can be, to a certain degree, affected by extrinsic agency. Thus pressure or attraction will cause permeation, where it would not otherwise take place, as when a single gas or liquid travels not only through, but beyond a membrane, where there exists nothing to imbibe it, which it would not do, unless subjected to propulsion. Electricity, possessed of hydragogue powers, acts on water in a similar manner, causing it to collect on either side of an animal membrane, at pleasure, although no other liquid is there to receive it.

9th. The penetrativeness of gases for each other seems to vary in velocity, but not in force.

10th. Reference to the above-mentioned laws and modifying agencies enable us to explain many phenomena hitherto imperfectly understood. We, by means of them, comprehend the uniform constitution of mixed gases in any vessel, or in the atmosphere, notwithstanding the greatest difference in specific gravity. It explains the diffusion of odours, the nature and power of spontaneous evaporation, and the probable nature and progression of caloric under slow conduction. It affords us new views of the theory of respiration, and accounts, in that process, for some well ascertained facts, for which there previously existed no adequate explanation.

It shows us how emphysema and tympanitis may happen without secretion of gases, or lesion of tissue, and how a spontaneous cure may be produced. It leads to the probability of the existence of gaseous matter of very various kinds, in almost every part of the animal frame, resident there molecularly, and not *en masse*, but susceptible of being collected into mass in the great cavities or the cells of the tissue, or the blood-vessels, by mechanical or electrical influence, or the attractive interstitial agency of other masses of air.

It teaches the important truth, that water is the great general infiltrator and diluent, a knowledge of whose habitudes will be thus rendered both clearer and more useful.

Before closing my remarks, I am happy to be enabled to say, that a considerable number of my medical friends visited my laboratory, and saw for themselves the verifications of my statements. I solicited their observation both for the confirmation of my own impressions, and for the greater readiness of reception which the public always affords to facts which have appeared in a similar light to several different individuals of adequate judgment.

In my next I hope to present a table of the rates of penetrativeness of liquids for animal membranes. I hope also to ascertain the amount of force. On the relation of the respirable gases to the blood, and other liquids, I possess already many interesting facts, which will be then promulged.

Philadelphia, September 15th, 1830.

Since the foregoing paper was sent to the editor of this Journal, I have had an opportunity of reading M. Dutrochet's short essay, entitled "*Nouvelles Recherches, &c.*" In it, I find that the author has discovered his mistake, relative to the action of acids in general, but has fallen into one quite as important, respecting the agency of diluted sulphuric acid. He *now* considers it a *nullifier* of endosmose, instead of a promoter of exosmose, being not only itself inactive, but the cause of inactivity in other solutions. Feeling confident of the power of diluted sulphuric acid to receive as much water as the animal membrane could convey, I, in conjunction with Professor Finley, carefully repeated our experiments on that substance. In every case, where the solution exceeded 1°, (Beaumé,) it was adequate to the occupation of as much water as could be presented by the membrane. At 2°, 11°, and 25°, the acidulous liquid gave the same *rate* of aqueous infiltration, as did alcohol, ether, &c. A solution of sulphate of soda, at 11°, and at 3° Beaumé, and a solution of ammonia at 40° centesimal alcometre being infiltrable by water, at a rate not less than that of the animal membrane, of course, afforded, when compared with that liquid, exactly the same results. Although all these substances gave evidence of having been contemporaneously transmitted through the membrane, yet the quantity, easily appreciated chemically, was not so great as to make a sensible difference in the altitude of the column, whose rise represented the transmission of water. When, by the entrance of a considerable quantity of water, the acid was so far diluted as to intermingle with it more slowly than the membrane could present it, a rapid diminution of ascent ensued. At length, so little was received, as to barely compensate for the effect of gravitation. Finally, the diminished power of reception being below the effect of gravitation, the liquid descended again, and the two columns reached a common level. Seeing these causes of change, we can estimate the rate solely by observing the time taken to traverse a *short space*, and that immediately at the commencement of the experiment. Unless the less penetrant liquid be of much more power of reception than is actually necessary, its dilution soon destroys its adequacy, and lessens the apparant rate, just as in forming solutions, we perceive a great diminution of solvent power, as the point of saturation is approached. In addition, when both liquids are traversing the membrane at the same time, there is a progressive approach to a common state, favourable to repose. M. Dutrochet, therefore, by observing the effect of solutions of different strength, in a considerable length of time, (an hour and a half,) obtained results, not the act of the membrane, but of the solution—not the maximum effect of the tissue, but the constantly diminishing action on water of a gradually diluted solution. His results might, therefore, have been anticipated by calculation; for, as water dissolves less and less, in a given time, of any soluble substance, so a soluble substance acts on water presented to it, in a steadily declining ratio. When the demand for water is above the powers of supply through the membrane, the rate will be regulated solely by the water and membrane, and is the same for a great variety of substances. When the demand becomes less than the supply, the case is one of simple solution, with which the membrane, may be supposed to have no connexion. It is then acting the part of a still surface of water.

The following facts, ascertained at an early period of this investigation, will place this principle in a strong light, An inverted syphon, such as already described, was filled with atmospheric air, a portion of which, by placing thirty-four inches of mercury in the long limb, was confined in the shorter one. There being here the same gas on both sides of the membrane, the current set in the direction given by impulsion and the long column fell—

$\frac{3}{8}$ths of an inch	in 2 hours and 30 minutes, or 50 min.	per $\frac{1}{8}$th.
$\frac{3}{8}$ths more	in 2 hours and 39 minutes, or 53	per $\frac{1}{8}$th.
$\frac{3}{8}$ths more	in 2 hours and 26 minutes, or $48\frac{2}{3}$ds	per $\frac{1}{8}$th.
$\frac{1}{8}$th	in 1 hour and 1 minute, or 61	per $\frac{1}{8}$th.

$1\frac{1}{4}$ inch in the whole in 8 hours and 36 minutes.

At this period of the experiment, when the mercurial column stood two inches and a half lower, *proportionally*, than at the commencement, a vessel containing carbonic acid gas, was placed over the shorter limb. Immediately the long column began to rise—

$\frac{2}{8}$ths of an inch	in 20 minutes,	or 10 minutes	per $\frac{1}{8}$th.
$\frac{1}{8}$th more	in 10	or 10	per $\frac{1}{8}$th.
$\frac{1}{8}$th	in $12\frac{1}{2}$	or $12\frac{1}{2}$	per $\frac{1}{8}$th.
$\frac{1}{8}$th	in $37\frac{1}{2}$	or $37\frac{1}{2}$	per $\frac{1}{8}$th.
$\frac{1}{16}$th	in 60	or 120	per $\frac{1}{8}$th.

The column appearing stationary, was left nine hours unobserved, at the end of that time—

$\frac{13}{8}$ths were lost	in 9 hours,	or $41\frac{1}{2}$ min. per $\frac{1}{8}$th.
$\frac{5}{8}$ths	in 3 hours 21 minutes,	or $40\frac{2}{10}$ min. per $\frac{1}{8}$th.
$\frac{2}{8}$ths	in 1 hour 24 minutes,	or 42 min. per $\frac{1}{8}$th.

At this moment, the mercury came into contact with the membrane, all the gas being excluded.

The uniformity of descent, and the progressively diminishing rise are striking facts. It will also be observed, that the carbonic acid *seemed* to cease action, because of a weight of nearly thirty inches of mercury, whereas, in another experiment, sixty-three inches were readily driven upwards. We therefore easily perceived the cause of Dutrochet's mistake.

One other nullifier of endosmose is thought by Dutrochet to exist. A solution of hydro-sulphuret of ammonia at first quickened, and then totally arrested the motion of the fluid in the stem of his endosmometer; for which he accounts by supposing the final production of sulphuretted hydrogen in the solution, and the extinctive agency of *that*.

The great activity of gaseous sulphuretted hydrogen, on which Dutrochet made no experiments, led me to suspect that its solution was gifted with considerable penetrant power, and by thus counterbalancing the amount of penetrating water, appeared to act in arrest of motion, presenting just such a case as we witnessed when comparing together olefiant gas and arsenuretted hydrogen. For verification, a solution of sulphuretted hydrogen in water, was, by means of the inverted syphon, compared with water, and scarcely any motion observed. A similar solution, enclosed by an animal membrane, in a wide-mouthed bottle, was placed in a vessel of pure water, mouth downwards. In this instance the membrane gave no sign of inflection at first, but after several hours showed a slight bend inwardly. In both these cases the portion of liquid, originally clean water, when tested by acetate of lead, afforded the deep black precipitate, indicative of the presence, abundantly, of sulphuretted hydrogen.

In a second experiment, with a solution of sulphuretted hydrogen enclosed in a bottle, the water placed in the outer vessel contained the slightest trace of acetate of lead. Scarcely was the bottle immersed before the precipitation of the lead commenced. Finally, a solution of sulphuretted hydrogen in water was, by means of the inverted syphon, compared with alcohol confined in its shorter limb. In this instance, and in every repetition, the movement was manifested towards the alcohol, the rise of which showed that the penetrative power of liquid sulphuretted hydrogen is somewhat greater than that of water, and of course much greater than that of alcohol. These experiments were made with extraordinary care, because by them seemed to hang the fate of this whole question of principle. The whole doctrine of regular rate of penetration, &c. must fall to the ground if my trials had been confirmatory of the observations of M. Dutrochet.

The totally different results, as to the force of penetration, at which M. Dutrochet and myself have arrived, render necessary a few words of explanation.

It will be conceded that the fairest mode of estimating the force is when the liquid is fresh and the process just well begun. The altitude of the highest column of mercury which it can raise will represent its power, and that column should, if possible, be laid on it at once. In this manner I proceeded, and found that both bladder and gum elastic were broken by a column higher than sixty-three inches, although just before giving way, the column was rising. It could rise solely by the power of penetration, no other known agent of motion being present. But M. Dutrochet laying on a column less than sufficient, left his apparatus to raise that column for a day or two, until the process of elevation ceased. The height then reached he considered as representing the power of *endosmose*. The attentive reader will readily perceive in this plausible experiment, the same error which deprived the facts, as to time, of value. The solution had become diluted, and the water on the other side had become impregnated, and, independently in a great measure of the weight of the column, the causes of production of penetrating currents had ceased, and these beautiful experiments reported, not the weight which could be raised, but the time required by such a solution to distribute its qualities uniformly, or nearly so, on both sides of the membrane. Left in that state the column descends, thus evincing the cessation of penetration, not its *forcible* repression. This is well proved by his latest experiment, in which having raised a column of mercury by the penetration of water into a solution of gum Arabic to twenty-eight inches, and while still rising, he replaced the external water by a solution of gum Arabic, when an immediate descent was observed. The substitution of clean water again caused an elevation of the column.

On the whole, captivating as is the method, and elegant as are the experiments of this little volume of M. Dutrochet, it does not bring additional support to his doctrine of *endosmosis*. Yet whatever may be the issue of the experimental investigation to whose rigid scrutiny this most important subject is committed, the philosopher and physician can scarcely find language adequately to express the obligation, the high obligation, under which science has been laid by the elegant labours of M. H. Dutrochet. In him we discover the *punctum saliens* of a principle which is the master spirit of animal and vegetable motion, the ruling power of chemical science, the governing influence of atmospheric composition, the presiding genius of respiration, circulation, and nutrition, the cause of disease, and the restorer of health. But whatever may be *now* his fame, how little is it compared to that which may be anticipated for him by one who takes even a careless view of the mighty field of novel observation, just redeemed from the rich wilderness of nature. This tribute is paid the more unhesitatingly because it is due, and because I have so freely criticised and censured where the cause of science and truth demanded severity. It is in great men, and in great discoveries, that blemishes are most ungraceful and most injurious. The very magnitude and extent of the principle for whose detection we must thank Dutrochet, give a fearful importance to the slightest co-extensive errors.

September 18*th*, 1830.

EXTRACT

FROM THE

AMERICAN JOURNAL OF THE MEDICAL SCIENCES,

FOR NOVEMBER, 1830.

Nouvelles Recherches sur l'Endosmose et l'Exosmose, suivies de l'Application Expérimentale de ces Actions Physiques a la Solution du Problême de l'Irritabilité Végétale, et à la Détermination de la Cause de l'Ascension des Tiges et de la Descente des Racines. Par M. Dutrochet, Correspondent de l'Institute, &c. 8vo. pp. 106. Paris, 1828.

This interesting little volume is another proof of the great industry and talent of its gifted author. He says—

"By these new researches the phenomena of *endosmose* and *exosmose*, which I have discovered, belong to a new order of physical events; and their potent influence on vital actions is placed beyond all doubt."

The most prominent points in the present treatise, are the discovery of liquids which are not penetrant, and of solids which are impenetrable, the development of the fact of difference of velocity with greater accuracy than before, and the attempt to measure and calculate the force of endosmose, and the difference of force exerted by different liquids.

In attempting to estimate the *velocity* of *endosmose*, M. Dutrochet placed, in a graduated endosmometer, solutions of sugar of different degrees of strength, estimated the specific gravity, and observed the number of degrees traversed in a given time. Taking the difference between the strength of the syrup at the beginning and end of each experiment as the mean specific gravity, he concluded that the velocity varies as the excess of the density of the syrup above that of the water. If we say that the difference shall be called 1, and the altitude 1, for the experiment with the weaker solution—a stronger one afforded a difference of 1.76, and an altitude of 1.74—a still stronger, a difference of 2.77, and an altitude of 2.71.

In another series the differences being as 1, 1.59, and 3.19, the altitudes were 1, 1.65, and 3.17, respectively.

In estimating the *force* of *endosmose*, syrups of various densities were placed in an endosmometer with two curves, so that its wide mouth looked downwards, and its narrow one upwards. The ascending stem, made very long, contained a column of mercury, the cessation of the ascension of which limited the height of the column supposed to represent the utmost force of the syrup at the moment of cessation. Syrups, whose densities were 1.085, 1.053, and 1.110, supported columns of 10, 22 10-12, and 45 9-12 inches, respectively. The difference of density compared to clean water, was 0.025, 0.053, 0.110, or, proportionally, as 1, 2.12, and 4.4. Now the columns of mercury were 10, 22 10-12 and 45 9-12, or 1, 2.19, and 4.48.

Or,	1	1
	2.12	2.19
	4.4	4.48.

At first sight these neat experiments seem conclusive, but on reflection we perceive that two important considerations have been left out of view, viz. the amount of change in the water of the reservoir, and the effect of gravitation on the exosmose. The force necessary to prevent *endosmose* can be known only by ascertaining what altitude of the mercurial column will prevent the saccharine solution from losing specific gravity, an experiment not yet made.

In M. Poisson the electrical theory of our author has found an opponent so able as to shake the confidence even of Dutrochet himself, who still clings, however, to a modification of his original doctrine. Poisson refers the power to the conjoint influence of capillary and chemical attraction, a position only erroneous because not adequately comprehensive. The membranes undoubtedly act by a power dependent on porousness, while the further transmission may be caused by a similar moleculo-porous power, by electricity, by pressure, chemical action, &c. &c.

A reference to the following table will strike the reader as bearing curiously on the question of cause.

Liquefaction of Gases by Pressure		*Rate of Penetration by Gases.*		*Quantities of Gases Absorbed by Charcoal.*	
Gases.	Pressure in Atmospheres.	Gases.	Minutes.	Gases.	Volumes.
Cyanogen -	4	Ammonia -	1	Ammonia -	90
Ammonia -	6½	Sulph. Hydrogen	2½	Sulph. Hydrogen	55
Sulph. Hydrogen	17	Cyanogen -	3¼		
Carbonic Acid	42	Carbonic Acid	5½	Nitrous Oxide -	40
Nitrous Oxide -	52*	Nitrous Oxide -	6½	Carbonic Acid	35
		Olefiant - -	28	Olefiant - -	35
		Hydrogen -	37½	Carbonic Oxide	9.42
		Oxygen - -	113	Oxygen - -	9.25
		Carbonic Oxide	160	Nitrogen -	7.5
		Nitrogen -	195†	Hydrogen -	1.75‡

Making the usual allowance for the inaccuracy of difficult experiments, there seems a probability of some relation not yet fully developed between the mechanical condensibility of the gases and their absorption by charcoal and other porous bodies, and between the latter property and the transmissibility through membranes.

Want of space, together with the large allotment already made for this subject in the present number, precludes further notice of the highly interesting little volume before us. It is worthy of the careful perusal of every physician who desires to look beyond the mere routine of our yet too empirical profession.

Last of the practical arts to adopt the Baconian system of improvement, its progress seems at length to yield promise of a brighter era, when founded firmly on the principles of nature and truth, and constituted of materials, drawn from the quarries of experiment and observation, rendered brighter by arrangement, and stronger by combination, the science of medicine will place the physician on that proud summit from which the astronomer and mechanician look down on the feeble systems of former times. It is only thus, by divesting medicine of its empiricism, its mystery, and its hypotheses, that we shall be entitled to claim for our art, its proper rank among sciences, and demand for the educated physician, that respect which he now is doomed to share with nurses and charlatans.

J. K. M.

* Philos Trans. 1823, Faraday.

† This Journal, present Number, p. 40.

‡ Saussure, 6th Vol. Ann. of Philos.